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Borrowing and shift-induced interference: Contrasting patterns in French–Germanic contact in Brussels and Strasbourg

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Summary

(The reader is referred to the previous issue of *Bilingualism* for the full text of the article.)

The aim of the present article is, in the first place, to test hypotheses derived from the model for contact-induced language change as formulated in Thomason and Kaufman (1988 et seq.) (henceforth T&K). The framework can be shown to predict correctly the basic asymmetries of the contact patterns in Brussels and Strasbourg, and is thus an invaluable tool for describing these patterns.

In the second place, the article aims at showing how an analysis of the similarities and the differences between two language-contact situations can contribute towards a further understanding of variability in this domain of research. More specifically, a comparison of the language-contact phenomena in Strasbourg and Brussels can shed light upon the debate around the nature of the constraints on contact-induced change. T&K (1988, p. 35) take a clear point of view in this discussion when they say "it is the sociolinguistic history of the speakers, and not the structure of their language, that is the primary determinant of the linguistic outcome of language contact". This article shows that despite the differences in the sociolinguistic situation of Brussels and Strasbourg, the overall contact patterns are very similar, both from a quantitative and from a qualitative point of view. Thus, the present article provides some evidence for the view that it is the structure of the languages involved rather than the sociolinguistic history of the speakers, which determines the outcome of language contact in the first place.

Brussels and Strasbourg form a very interesting test case for T&K's model, because the authors do not discuss these contact situations in their book at all. This makes it possible genuinely to test the predictions of the model against the data available from Brussels and Strasbourg. T&K's model provides a framework within which the results of a large number of descriptive studies about language contact in Belgium and Alsace can be summarised and evaluated. To my knowledge no efforts have been made until now to compare the mutual influences in these areas, apart from the analyses presented in Treffers-Daller (1995). The article thus wants to contribute towards a more general perspective on language contact along the Romance–Germanic language frontier, focusing on the contrastive effects of two types of linguistic influence which T&K have called "borrowing" and "shift-induced interference". "Borrowing" is defined as the "incorporation of foreign features into a group's native language: the native language is maintained but is changed by the addition of the incorporated features" (T&K, 1988, p. 37). Shift-induced change, on the other hand, "results from imperfect group learning during a process of language shift" (T&K, 1988, p. 38).

The contrastive effects of borrowing and interference distinguished above can very clearly be shown in Brussels and Strasbourg, where the influence of the Romance varieties on the Germanic varieties is mainly visible in the lexicon, and the Germanic varieties have primarily exerted influence on the phonology and syntax of the Romance varieties. The analysis is based on a comparison between the French–Dutch corpus described in Treffers-Daller (1994) and the Alsatian–French corpus described in Gardner-Chloros (1991).

The article gives a detailed overview of the main similarities and differences between the sociolinguistic situation of Brussels and Strasbourg. The language contact situations in these cities are comparable because in both cities French is in contact with a less prestigious Germanic variety. In both cities bilingualism is asymmetrical in that the speakers of Alsatian and of Brussels Dutch are more likely to learn French

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than vice versa. Contrary to what I have claimed in Treffers-Daller (1995), however, I believe that from a sociolinguistic point of view these two cities are only superficially similar. A careful analysis of the facts shows that there are major differences between Brussels and Strasbourg on a number of points that T&K consider to have predictive value for the amount and the depth of borrowing or interference one finds in a speech community, namely: a high level of bilingualism, length of contact, relative sizes of speaker populations and socioeconomic and/or political dominance of source-language speakers over borrowing-language speakers. T&K also point out that well-established standard dialects of the borrowing language can hinder structural borrowing to some extent.

The analysis given in the paper shows that the language contact patterns in Brussels and Strasbourg are very similar. Brussels Dutch and Alsatian mainly borrow lexical items from French, whereas structural borrowing is limited to relatively minor features. The basic syntax of Alsatian and Brussels Dutch has remained unaffected by French. Phonological, morphological and syntactic influence from French is mainly visible in the words borrowed from French.

For Brussels French and Alsatian French, the opposite is true. Lexical borrowing from the Germanic varieties is less important in the French varieties than structural influences from Brussels Dutch and Alsatian. The Germanic varieties have exerted lexical influence on French through loan shifts (Weinreich, 1953) rather than through lexical borrowing. Phonological influence from the Germanic varieties in French is not confined to lexical borrowings from Alsatian and Brussels Dutch. Thus, whereas structural interference in the Germanic varieties is clearly linked to lexical borrowing, structural interference in the French varieties is not connected to lexical borrowing at all. This confirms T&K's (1988, pp. 114–115) prediction that "while borrowed morphosyntactic structures are more often expressed by actual borrowed morphemes, morphosyntactic interference through shift more often makes use of reinterpreted and/or restructured TL morphemes". These asymmetries can be predicted and explained with the help of T&K's framework for contact-induced change. It is well known that many speakers of Alsatian and Brussels Dutch have experienced language shift in the direction of the prestige language French. As a matter of fact, there remain few monolingual speakers of either Brussels Dutch or Alsatian. As a result of the process of language shift, French as spoken in Brussels and Strasbourg is typically marked by substrate (and adstrate) influence of the Germanic varieties. This influence becomes apparent in phonology and syntax rather than in the lexicon, as predicted by T&K.

It is quite remarkable that lexical borrowing is restricted to T&K's level 2 (and some aspects of level 3) in Brussels and Strasbourg. Given the fact that the inhabitants of both cities have experienced strong cultural pressure from French, one would have expected to find more intimate forms of borrowing. T&K suggest that the presence of a standard language related to the borrowing language may constrain the borrowing process. In the Brussels case, this is not unlikely, as standard Dutch has gained importance in the city. In Strasbourg, on the other hand, standard German hardly plays a role in everyday life. Thus, it is unlikely that the presence of a standard language has had a constraining influence on the borrowing process in Strasbourg.

A comparison of the situation in Brussels and Strasbourg with the situation in South Tyrol and in East Belgium shows that the outcome of language contact is very similar in all these areas. According to Riehl (1996) borrowing is also limited to level 2 in the local varieties of German spoken in the Austrian south Tyrol (in contact with Italian) and east Belgium (in contact with French). There are only incidental examples of phenomena belonging to level 3 in those areas. This means that lexical borrowing found in a number of Romance–Germanic contact situations is very similar, despite the sociolinguistic differences between the speech communities. This indicates that sociolinguistic factors may not have been the crucial determinant in the borrowing process in Romance–Germanic contact.

The conclusion of the paper is that it is hardly possible to explain the similarities in the contact patterns with the help of macro-sociolinguistic factors. The analysis given here should not be interpreted as a claim that sociolinguistic factors do not have a bearing upon language contact at all. It is evident from the literature that the factors discussed here as well as micro-sociolinguistic factors do have an influence on the type and frequency of language contact. The point I have tried to make here is that sociolinguistic factors are of little help in explaining the similarities between the contact patterns in Strasbourg and Brussels. The similarities in the outcome of language contact find a plausible explanation in the fact that the two language pairs involved in the contact settings are very similar if not identical. Thus, the facts described in this and previous papers (Treffers-Daller, 1997 and in press) lend support to the claim that structural factors rather than sociolinguistic factors are the primary determinants of the linguistic outcome of language contact.

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PEER COMMENTARIES

Update on spoken French

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My comments are restricted to one very small but possibly significant area which is raised under the French varieties on page 17. Jeanine Treffers-Daller (JTD) quotes De Vriendt (1988), who claims that an example of the Dutch influence in Brussels French is in the fronting of direct objects. The following example (24) is given:

Dix francs moi je donne

As this structure is not dissimilar to utterances made by speakers in my own corpus of spontaneous French spoken in hexagonal France, it was felt that a brief aside on the topic might be of interest, especially in view of the claim made by JTD (p. 19) that "Lexical borrowing from the Germanic varieties is less important in the French varieties than structural influences from Brussels Dutch and Alsatian." The example of fronting of direct objects forms a key plank in the argument which ensues concerning the asymmetrical nature of borrowings, to the effect that the "lower language" will adopt vocabulary items from the "upper language" (indeed, as T & K, 1988, p. 37, quoted by JTD, p. 19, say. "in a borrowing situation, the first foreign elements to enter the borrowing language are words"), whilst the "upper language" adopts not only phonology but also structural items, though no vocabulary. It seems implausible that the French varieties should adopt structural items without first adopting lexical items, and JTD's thesis is undermined by the contentious nature of the examples given to support the argument that Dutch syntax influences Brussels French. It is only if spoken data from contact situations are compared with French spoken data that we can discern whether the French variety is indeed influenced by Dutch or, on the other hand, whether the features discerned are characteristic of spontaneous varieties of French everywhere.

One problem arises in the dearth of description of spontaneous spoken French. Without a reliable guide to the nature of standard French, how can we decide whether a variety diverges from it? It is only relatively recently that it has been suggested that the grammar of the spoken language may be different from that of the written language. Brazil (1995) suggests for English that we need to reject the concept of "sentence grammar", and proposes an exploratory grammar based on process, not product lines (Halliday, 1985), making the distinction "writing exists, whereas speech happens". SVO syntactic arrangements fall victim to communicative purpose which combines intonation with a significant degree of liberty with regard to word order and old-fashioned grammatical accuracy. In spontaneous communication, Halliday's concept of theme/rheme overrides considerations of word-class in the syntagmatic ordering or "chaining" of elements in discourse. Brazil talks of a linear grammar which is "purpose-driven" rather than "sentence-oriented" (1995, p. 4).

French researchers, too (e.g. Blanche-Benveniste and Jeanjean, 1987, p. 43), have suggested that the theoretical frameworks within which French linguists are working have not been extended in such a way as to cope adequately with the features which characterise spoken French:

> Au total, les linguistes de ces années récentes n'ont pas proposé de cadre pour absorber le français parlé. Ils l'ont souvent vu comme un secteur marginal, qu'on ne peut pas intégrer à une grammaire qui serait celle du français "commun, de base, de référence".

Culioli (1983, p. 295) goes so far as to suggest that the written form provides a stability and coherence which is threatened by the apparent chaos inherent in the spoken language:

> La culture française est une culture puriste … La langue écrite y est outil de cohérence; elle nous fournit la sécurité des formes stables, fixées et normées; elle est un facteur d'unité …

If the written language represented the norm, the pinnacle of rational thought, the spoken language was considered to fall well short of these Cartesian ideals:

> car le français parlé était une succession d'amorces avortées, de ratages, de phrases en suspens, qui paraissent défier l'analyse. En un mot, le français parlé était du français mal tourné, qui tournait mal. (Culioli, 1983, p. 291)

Blanche-Benveniste and Jeanjean (1987, pp. 155ff.) talk interestingly of the "avant-texte de l'écrit" and compare the drafts leading to Proust's "Longtemps je me suis couché de bonne heure …" with hesitation and reprise phenomena in the spoken language. Blanche-Benveniste and Jeanjean (1987) quote Culioli (1982, p. 10) where he says that:

> Rares sont à ce jour les efforts pour construire une linguistique des opérations du langage qui se préoccupe du travail énonciatif et qui ne ramène pas l'activité signifiante à l'empaquetage codé d'une intention claire et arrêtée, une linguistique des ajustements énonciatifs et non une linguistique de la communication manifeste et réussie, où l'on ne programme que des prédications heureuses.

Spoken French has been the object of a number of studies in recent years – see Ambrose (1996) for a useful bibliography – and a number of spoken corpora have been and are being established. The GARS (Groupe Aixois de Recherches en Syntaxe) has been most active in this respect. Bureau (1988) and Sabio (1995) look at hesitation

and backchannelling phenomena but do so from a syntactic point of view, attempting to classify the different types of "arrêts et retours" and "compléments antéposés" respectively, elements which do not fit easily into a syntactic description. They do not, however, integrate these elements into an overall theory of discourse structure motivated by sociolinguistic and pragmatic considerations.

Brazil's thesis that a linear approach to interpretation is required is supported by Danon-Boileau and Morel (1994)'s telling example:

Non mais moi, question saumon, pour la pêche, l'Ecosse, tu vois, c'est ce que je préfère

As explanatory procedures the concept of left and right dislocation are inelegant by comparison with Brazil's linear grammar, positing as it does that language is driven by a communciative purpose. Danon-Boileau and Morel argue convincingly that the spoken language creates schemata which are successively redefined, thus narrowing the scope of the theme. Once the theme is adequately defined, the rheme follows.

The approach suggested by Danon-Boileau and Morel (1994), in which suprasegmental factors, a system of ligateurs énonciatifs et discursifs and the concept of theme and rheme are included along with syntactic features, and where the concept of left-detachment or "prefixing" is abandoned, opens more promising avenues in finding a theory which may explain some of the features of spoken French.

Barnes (1985, p. 37) claims that in her corpus "moi" accounted for 43 per cent of the total number of left detachments. "Moi" in this position has been variously interpreted as a topic establishing device, a turn-taking device or means of making a comparison or giving an example or illustration, more traditionally, of providing emphasis. Barnes also claims (1985, p. 45) that "the other frequent type of Pro-detachment is that with demonstrative ça. Out of a total of 139 tokens, 115 are with the verb être."

The fact that the term "left-detached" inappropriately imposes a TG framework on the grammar of the spoken language has been highlighted by Carter and McCarthy (1995), prompted by Fawcett. The data from this corpus of spontaneous hexagonal French confirms his argument that "left-placed or fronted items of this kind are perfectly normal in conversational language, and are quite within their 'right place'" (Carter and McCarthy (1995, p. 149). Such theories may also explain fronting such as the direct object fronting exemplified in the Brussels French data provided by De Vriendt (1988).

Other examples of direct object fronting without the double marking which JTD quotes from Blanche-Benveniste are to be found in the literature. Gadet (1989, pp. 170–173) draws a distinction between clitic reprise, cleft structures and binary constructions, of which she gives the following two examples:

les maths en terminale, y a intérêt à s'accrocher
la cantine, on n'a pas à se plaindre

It is thus not only in Brussels French as JTD claims that "many different constituents can occupy the first position in the sentence" (JTD, p. 18). She goes on to say that in the majority of examples it is ça that is fronted. In the following examples drawn from my own corpus of spontaneous French, we see that the same principle applies, though the grammatical function of ça could not always be said unequivocably to be that of a direct object:

Moi, ça, ça, ça j'avais déjà préparé à l'avance pour qu'on ait le temps de bavarder

Moi, ça, je supporte pas

On a dû se battre pour la France aussi. Ça on a jamais refusé, quoi, mais euh moi, je tiens compte de la Bretagne

Nous aimons la propriété. Ça, c'est pas de doute, hein

Alors y en a un qui, alors le garçon prend une autre fille, la fille prend un autre gars quelquefois c'est pire, quelquefois c'est un peu mieux, mais on recommence. Ça, les enfants ne sont pas heureux du tout du tout. Non

Oui, ben, ma femme a laisser tombé ses deux enfants. Ça j'aimerais mieux pas qu'on . . .

Ça là je sais bien vis-à-vis du gouvernement pour faire audit sur l'échec scolaire

Ça bon je sais pas si je suis autoritaire

In many of these examples, ça is used as a generic referent both anaphorically and cataphorically to "gather" the subject into one place, acting as a pivotal point around which the new information can be organised. The beauty of ça lies in the flexibility of the syntactic connections which it can form, facilitating the on-line processing required for rapid speech.

The linear and purpose-driven nature of the paratactic discourse developments which characterise spontaneous spoken French may convincingly be put forward as an alternative explanation for the fronting of the direct object suggested by De Vriendt as an example of the Dutch influence in Brussels French.

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The nature of constraints on contact-induced change

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Jeanine Treffers-Daller (henceforth JTD) must be congratulated on what is perhaps the first explicit attempt to verify empirically Thomason and Kaufman (henceforth T&K)'s (1988) influential claim that the sociolinguistic history of speakers outweighs linguistic structure in determining the linguistic outcome of language contact. Based on quantitative and qualitative comparisons of the effects of contact between French and Dutch in Brussels and Strasbourg, the similarities she detects, despite differences in the sociolinguistic situations, lead her to conclude that the primacy of sociolinguistic history is not supported.

Despite the merits of the paper, there are at least two methodological reasons why its conclusion remains ultimately unconvincing. One involves the difficulties in operationalizing T&K's model. T&K had already recognized that in the absence of precise information about how individual social factors conspire to determine the relationship between languages in contact, a clear prediction about the outcome (in terms of what they call "borrowing" and "shift") is impossible to make. Yet the factors pertinent to identifying the "sociolinguistic history" of a speech community are neither made explicit by T&K, nor operationalized by JTD.

Thus, although a number of differences in the relationship between languages in Brussels and Strasbourg are detailed, it is not clear that these are in fact relevant to their respective sociolinguistic histories. A concrete theory of power differentials in bilingual speech communities would help establish that the situations differ in ways which are demonstrably significant and relevant to the model. Giles's (1979) notion of ethnolinguistic vitality of minority and majority language groups is one such theory. Ethnolinguistic vitality can be evaluated as the sum of several different social factors, including demographics, that is, minority versus majority status; institutional support, that is, official status and educational opportunities; as well as intergroup attitudes, as measured by conflict. All of these factors are cited by JTD.

Once a notion like ethnolinguistic vitality is invoked as a metric, differences between Strasbourg and Brussels with respect to language dominance diminish. Indeed, from this perspective, one could equally well argue for similarities in their sociolinguistic histories. For example, factors detracting from the ethnolinguistic vitality of Dutch in Brussels, such as its minority status, may be offset by institutional support in the schools and media. Conflict with francophone groups could strengthen its role as a contrastive symbol of ethnic identity. Although institutional support and the conflict associated with linguistic expression of ethnic identity are less evident in the case of Alsatian, its vitality is probably maintained by the sheer numbers of its speakers. Moreover, in both cases French is undoubtedly the more powerful language, whether measured in terms of overall number of speakers, differences in official status or recognition worldwide. Thus, evaluation of differences between communities depends on the characterization of sociolinguistic history. A sine qua non for determining the relative contribution of social versus structural factors to contact-induced change would seem to be a clarification of the social factors involved and their significance.

JTD's claim for the primacy of linguistic factors is further challenged by findings consistent with T&K's claim showing that the typological characteristics of the contact languages are not major predictors of the outcome in other speech communities. An early comparison of a French–English bilingual community in Ottawa with a Spanish–English community in New York showed clear differences in the types of language mixture, despite the typological similarities in the contact languages (Poplack, 1985/1988). Studies of contact between Arabic and French (e.g. Na'it M'Barek & Sankoff, 1988; Sankoff & Na'it M'Barek, 1990) have confirmed that patterns of combining the same two languages may differ dramatically from community to community, a result that can only be attributed to factors of an extralinguistic nature. Perhaps the strongest evidence of the role of sociolinguistic history comes from Adalar and Tagliamonte's (1998) study of Turkish–English bilingualism, involving both the same speech community and the same language pair. Even here, patterns of language mixture vary according to speaker generation and sociolinguistic situation, showing that differences in bilingual behavior can be directly linked to differencies in the social situation of speakers. A claim for the primacy of linguistic factors in contact-induced change should account for these other results.

Even admitting that JTD's characterizations of the sociolinguistic differences between Brussels and Strasbourg are both pertinent and correct, there remains another reason why this study is not an empirical test of T&K's theory. T&K presented a framework for contact-induced language change; JTD's invocation of synchronic linguistic evidence only (e.g. nonce borrowing, codeswitching and interference) is of uncertain relevance. While the lexical and structural outcomes she cites are undoubtedly the result of language contact, we do not know which, if any, result from change, contact-induced or otherwise. For example, included in counts of borrowed items are both attested loanwords and other-language items which are ambiguous as to borrowing or codeswitch status. JTD does not

distinguish among them, since this does not affect the distinction she wants to test (between lexical and structural outcomes). However, these phenomena are differentially related to change. Attested loanwords may be argued to have altered the lexicon, but no evidence is provided, in apparent or real time, that the ambiguous lexical items (whether considered nonce borrowings or codeswitches) have changed the languages in which they are temporarily incorporated.

Thus, despite its important contributions on the synchronic comparative level, this study falls short as an empirical test of T&K's model, and by extension, as a predictor of the effects of specific types of language contact. The sociolinguistic history of a contact situation cannot be characterized by listing the appropriate social factors, no matter how exhaustively, but rather requires a more complex analysis of the social situation and possible interaction between competing factors. Before we can assess the effect of sociolinguistic history on contact-induced change, we require a more precise definition of the notion of sociolinguistic history and a method for calculating its value, as well as knowledge of whether the linguistic phenomena adduced as evidence in fact result from change. Studies such as this one are a valuable first step.

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But sociolinguistic factors do matter!

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I have been convinced that there are significant similarities in the kinds of language contact phenomena that exist in the Germanic–French linguistic frontier cities of Strasbourg and Brussels. However, I am not convinced that "sociolinguistic factors are of little help in explaining the similarities between the contact patterns in Strasbourg and Brussels. The similarities in the outcome of language contact find a plausible explanation in the fact that the contact situations are typologically similar".

To take the point about typological similarity first, let us assume for the sake of argument that Brussels Dutch and Alsatian are structurally similar enough to allow us to call the contact situations "typologically similar". Even so, there is little or no evidence offered of specific typological factors which are producing the observed outcomes. What aspect of the typologies of Alsatian and French favours borrowing from French into Alsatian, but not vice versa? What aspects of French and Dutch syntax favour influences from Dutch on French syntax, but not the reverse? While it is not impossible to think of morphophonemic constraints which might cause, say, an isolating language to resist borrowing from a polysynthetic one, no such principle seems to be operating here. In fact, the problem with basing a claim on this particular set of examples may be that French, Dutch and Alsatian are all typologically similar in significant ways. Even where there are apparently categorical differences, such as the ordering of adjectives with respect to the nouns they modify, there is often an alternative or variant construction which makes it difficult to make any absolute claims – so for example, "in French many adjectives can occur prenominally ... especially if one wants to obtain special stylistic effects".

What about the relevance of macro-sociolinguistic and historical factors? In fact, we are already told that these account for some of the similarity of outcomes in terms of Thomason and Kaufman (henceforth T&K)'s model. The structural influences on French are the result of shift-induced change – surely an aspect of the "sociolinguistic history" of the speakers – while the lexical borrowing results from language contact which does not have to involve a language shift. The main thing still to be explained is the asymmetry in lexical borrowing (nearly all from French into Germanic) and structural influences (nearly all from Germanic on French). As the author puts it, "It is quite remarkable that lexical borrowing is restricted to level 2 (and some aspects of level 3) in Brussels and Strasbourg. Given the fact that the inhabitants of both cities have experienced strong cultural pressure from French, one would have expected to find more intimate forms of borrowing" (page 20). One problem with this statement is simply that we do not have a clear guideline as to what degree of "cultural pressure" is needed to produce structural borrowing. Given that shift-induced change, in T&K's model, "begins with sounds and syntax", while lexical borrowing characterises the lowest level of a "borrowing situation", all we need say is that the degree of language contact or intimacy in both the Brussels and the Strasbourg case is of the first level, with some developments in the second.

Unfortunately for all of us who would like to find general principles governing the phenomena of language contact, language contact situations rarely come simple; several things are nearly always going on at the same time. In the cases under consideration, we not only have language shift with attendant structural changes which result from interference, but a fairly stable situation of bilingualism with both the "old" and "new" languages continuing to be spoken, though with status differences. Furthermore, in societies where schooling and literacy reach close to 100 per cent of the population, it would be a serious omission to overlook the role of standard languages and literacy. The fact that, in Alsace, Standard German is not important as a spoken variety may be less important than the fact that written Standard German has a presence there, though a diminishing one. Meanwhile the presence of written French is pervasive everywhere in daily life both in Alsace and in Brussels. This alone could account for the asymmetric large-scale lexical borrowing of French words into Alsatian and Dutch.

What kind of model might help us to decide on whether language contact is "intimate" enough to produce higher levels of borrowing? Le Page's concepts of focusing and diffusion may be useful here. In a language community where there are diffuse linguistic norms, there is little pressure on speakers to conform linguistically: there is no penalty to be paid for variation from a norm. Variability is both possible and expected, and not commented upon adversely. In a language community where norms are relatively focused, there will be "greater regularity in the linguistic code, less variability" (Le Page and Tabouret-Keller, 1985, p. 116). Where norms are more focused (whether on a prestige norm or a non-standard norm) speakers will have less freedom. In a contact situation we would expect that highly focused norms, if they already exist, will impede borrowing, at least at some level. This seems very clear in the case of French, where resistance to lexical borrowing takes the form of purism. However, non-standard language can also be highly focused, as shown by the work of Milroy (1987). Such focusing may be especially likely where a language variety is important as a symbol of group identity – as is the case with Alsatian in particular. While non-standard varieties may be susceptible to lexical

borrowing as a result of cultural dominance, it may be that focused community norms resulting from group solidarity make them relatively resistant to structural influences.

We might hypothesise that in a language community with sufficiently diffuse norms, borrowing can proceed with relative freedom in the absence of pre-existing focused norms. Speakers will be able to borrow at the lexical, morphological or syntactic level without suffering any sanction. They will not be denounced as "wrong" or ridiculed. Under these circumstances language systems may almost merge without any of their speakers worrying about it.

To some extent, linguistic focusing and diffusion are products of a society's world-view, of its beliefs about itself, of its cultural norms. They are not completely predictable from its history or the nature of its social organisation. However, it seems we are more likely to find diffuse norms where formal education is not widespread, where literacy (at least literacy on the European model) is limited, where languages are not sharply distinguished in terms of status, where learning of languages as well as learning more generally is informal or family-based. Prevalence of institutionalised schooling, literacy and a hierarchy of prestige attached to languages will favour focused norms and relatively "impermeable" language boundaries.

Both the situations described in the article under discussion in fact belong to the second type of society. We would expect a relatively high degree of focusing, which will act as a brake on structural borrowing. Lexical borrowing appears to follow its own rules to some extent.

To conclude, I am wholly in favour of comparative research of this type which ultimately will tell us more about the nature of language itself as well as of language contact. However, if typological factors are what have prevented "more intimate borrowing", I would like to know exactly what form these take. Typology is a particular bone of contention in language contact studies. Among creolists, for example, one group will see striking typological similarities among certain creoles or between creoles and putative substrate languages which "proves" their relatedness; another group will deny these typological similarities or attribute them to chance. Further research seems to be indicated to develop typological tools which will be useful in analysing language contact. In addition, I think that there are two important sociolinguistic factors which must not be overlooked. One is literacy: even a focus on the spoken language cannot allow one to discount the nature of literacy in a community where almost everyone reads and writes on a daily basis. The second, probably related to the first, is the nature of the community attitudes to language and the kinds of norms which exist there, whether focused or diffuse. This concept, I believe, has a role in explaining the kinds of language-contact phenomena which may develop.

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A tale of two cities: A response to Treffers-Daller

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Treffers-Daller (henceforth JTD)'s empirically rich comparative study of the patterns of contact in Brussels, Belgium, and Strasbourg, France, is extremely difficult to respond to. Part of the difficulty resides in the fact that although both the majority of the author's own conclusions and the data adduced to support them provide an overwhelming endorsement of the central thesis of Thomason and Kaufman (henceforth T&K), the author wishes, she tells us, that her contribution be seen, at least in part, as a serious challenge to that thesis. As someone who is on record as having argued, sometimes perhaps too vigorously, against standard sociolinguistic analyses of language contact (against because they generally ignore grammar), I have no sociolinguistic axe to grind. I am, however, unable to see how her findings or her arguments support the case she wants to make.

Despite what some may legitimately see as an autonomist or structuralist bias in my own work on contact to date, I have never been able seriously to entertain the idea that anything other than political economy, the (extremely) superficial aspects of some of the consequences of whose real impact are sometimes, mistakenly in my view, referred to as (macro-) sociolinguistics, particularly (though, it seems, not exclusively) in North America, could ever explain the fact that Modern Hindi (MH), for example, has borrowed many, many more words from English than English has borrowed from it, or the fact that the second element in the so-called redundant compounds of MH, discussed in Singh (1982), is of Perso-Arabic (rather than, let us say, Dravidian) origin. It has always seemed clear that whereas political economy (or, if one insists, sociolinguistics) decides what must be learned, maintained, or curtailed, grammar (in all its three dimensions: universal, typological, and language-particular) decides what the learning, maintenance, or curtailment will have to work with or on, for the obvious, trivial reason that the outcome is invariably a language (notwithstanding the fact that some scholars add qualifying epithets to some of these languages). Linguistics (= structural considerations) can and does supply the materials with which languages about to be acquired or abandoned negotiate the paths to be taken by them, but it is the sociolinguistic history of speakers alone that decides the destinations (the outlines of whose linguistic maps are predefined) to which their speakers must take them. The latter chooses the actors that arrange the distribution of codes, the former the paths these codes must follow to pursue the sociohistorically driven political will or fate of their speakers – they must, in other words, expand, contract, or retreat along paths which similar winners and losers have followed in the past (and, one hopes and fears) will follow in the future. The latter decides which speakers will acquire a new language (or, to put it more correctly, which language will acquire new speakers), the former what adjustments they will have to make when acquiring it. Grammar, in other words, merely supplies the materials with which history (in the appropriate sense) plays the linguistic games it has defined the rules for. Those rules, of course, include some cooperation with grammar, but it is (sociolinguistic) history that holds all the trumps. The history of Sanskrit, Latin, and now English provide all the evidence one could ever want to hold on to this idea.

Such doubts as have cast a shadow on it have to do with claims that other sociofunctional considerations may make the appeal to grammar completely redundant, but no one, to the best of my knowledge, has ever claimed, for example, that speakers of SVO languages are destined to force speakers of SOV languages to abandon their linguistic ships, or that languages with ablauting morphology must self-destruct in favor of affixing languages as soon as they see them in the neighborhood (and this despite the intriguing correlation between the presence of the definite article in a language and the expansionist policies of its elite speakers!). I take T&K's (1988, p. 35) eminently reasonable and insightful claim that "it is the sociolinguistic history of the speakers, and not the structure of their language, that is the primary determinant of the linguistic outcome of language contact", to mean just that. Whether French in Quebec, for example, survives or not is a matter of the political economy of Quebec within the contemporary (economic) world-order rather than a matter of, for example, the fact that this external sandhi Romance language is in contact with a Germanic language that does not like external sandhi very much. Political histories of speakers are, in fact, so powerful that they can even bring languages into existence out of what the linguist saw as the blue till yesterday, but is now constrained to write about them as if they were always there (e.g. the history of the language called Urdu, discussed in several places, including Cardona (1974), Rahman (1996), Rai (1984), Singh (1995), and Srivastava (1979)). More directly relevant is the fact that French in Ontario, Canada, has met, at least partially, the fate that seems to be reserved only for Germanic languages on the Romance–Germanic border on the Continent (not being on the Continent, England, presumably, does not count!).

Lest I should be accused of indulging in a priorism, let me take up the facts reported by JTD. She reports that whereas French "as spoken in Brussels and Strasbourg is typically marked by [phonological and syntactic – RS]

substrate (and adstrate) influence of the Germanic varieties'' (page 19; see also pages 17–18, which report the findings of De Vriendt, 1988) these latter show no such extensive influence from French (pp. 13, 16, 17 etc.). As for morphology, the French influence is even less (p. 14). Clearly, Dutch speakers have been obliged by the sociolinguistic history of Brussels and Alsatian speakers by the sociolinguistic history of Strasbourg to learn French, and they have learnt it the normal (for this sort of situation) way. Whereas some sociolinguistic interventions in Brussels have apparently chipped away a bit at the status of French there, in Strasbourg it seems to be marching on in unhampered glory. What French seems to have offered in return to Alsatian and Dutch is the ''opportunity'' to borrow a lot of words from it (as payment for partial language shift?). The speakers whom the relevant French speakers must consider (or must have considered until very recently) to be their ''country cousins'' have, of course, been ''allowed'' to keep their basic vocabularies (p. 10).

The similarities, noted and extensively documented by JTD, can be said to follow straightforwardly from the sociolinguistic observation that the French speakers in our two cities don't seem to be actively involved in learning Germanic varieties that would seem to be easily available to them. This sort of observation does not, of course, explain why only the non-basic words that have been borrowed have in fact been borrowed – that would require further socio-pragmatic investigations. It does, however, tell us why ''these asymmetries'' are symmetrical in our two cities.

JTD, to her credit, recognizes that all this follows from T&K (pp. 19ff). And yet she wants to claim that there is something wrong with T&K. What is wrong, she says, is that these similarities ought not to exist in two cities as ''different'' as Brussels and Strasbourg, which, it is useful to recall, were presented as rather similar by JTD herself in 1995. The only thing that needs to be added to her table of sociolinguistic differences between these two cities (Table 2) is the fact that whereas code-switching has more or less disappeared from Brussels (pp. 6–8), it has gained ground in Strasbourg, presumably an index of the fact that the recession of Dutch has been checked (and the maintenance of Alsatian not encouraged) by the powers that be. One would have thought that this was in fact a linguistic outcome of, rather than a difference in, the sociolinguistic make-up of these two cities.

Be that as it may, JTD's logic seems to be that given these differences, linguistic things ought to be far more different than they actually are. The fact that they are not (more different) suggests to her that after all perhaps one can hold on to some version of some structuralist hypothesis (*pace* T&K, obviously).

There are two major problems with her proposal. First, the prestige of French in our two cities and the consequent language-shift, despite some checks on it in Brussels, is almost enough by itself to explain almost everything she wants explained. Secondly, if grammar and typology are responsible for the similarities she finds, why are their effects restricted to the Germanic–Romance border on the Continent (or, conversely, why can't these results be generalized to all cases involving any Romance and any Germanic language)? JTD shows some awareness of the generalizability problem and offers a very brief comparison with the situations in east Belgium and south Tyrol. Her conclusion: ''This means that lexical borrowing found in *a number of* Romance–Germanic contact situations is very similar'' (page 20, italics mine). Apparently, there are cases where things are a bit different, and, apparently, even more so when non-lexical matters are involved. I regret that she does not report the findings of Riehl (1996) more extensively.

Perhaps her argument resides in quantities (her tables 3 and 4), but here the invocation of grammar may have to be weighed against the invocation of some still uncharted *(micro-) sociolinguistic factor to explain the asymmetries, such as the relative neglect of French adjectives in Strasbourg (p. 9). And even when it comes to similarities, the high frequency of nouns borrowed *from* French may not be much to write home about (both Indo-Aryan and Dravidian languages have borrowed a lot of nouns from English). As for the opposite direction, it is useful to note that both English–InterHindi and Indian English borrow interjections and nouns from Hindi (see Hooper, 1917 and Kachru, 1983). One would have to show that the similarities in the quantitative asymmetries can't be made to follow from the same *(micro-) sociolinguistic factors that seem to have colored the phonology of Brussels French and persuaded the Alsatian speakers to begin to transfer their identity to their mixed-code. The case that Germanic languages will always borrow nouns in the neighborhood of 60% and interjections less than .5% and that French will borrow Germanic interjections to the extent of about 50% and nouns only to the extent of about 30% is much harder to make, and has in fact not been made. Besides, what if comparable figures show up in contact-pairs that involve neither Germanic nor Romance? Needless to add that ''comparable'', ''in the neighborhood of'', ''about'', and ''approximately'' are very slippery expressions.

As any contact situation implies language learning/acquisition, it might be worth our while to summarize what we know about second-language learning vis-a-vis phonology, morphology, lexicon, and syntax. There is a general consensus in the field that interlanguages – languages T&K refer to as imperfectly learned languages – are typically colored by L1 phonology and syntax and not colored by L1 morphology (see Singh, 1991 and Singh and Parkinson, 1995 for extensive discussion). The reason is very simple: phonology and syntax are blind, not subject to manipulation without conscious knowledge and instruction. It is, in other words, in the nature of phonology and syntax to interfere. This imperfection can be overcome, but learners generally don't bother. Morphology and lexicon, on the other hand, are much more transparent, and are held at bay by the sheer desire to learn the second language. These component-specific propensities, combined with sociolinguistic histories of speakers – histories that place them in specific slots – seem sufficient to understand the outcomes of linguistic contact. Grammar, of course, plays a

role – it charts the path a language must take to get to where its speakers and their (sociolinguistic) histories have decided or have been forced to take it. There is, unfortunately, nothing in the structure of Germanic languages that predisposes their speakers to contribute a substrate to Romance languages they come in contact with, though they will, of course, generally not bother to voice word-final obstruents of any language they have been asked by history to learn, at least not initially. As for a Romance language contributing a substrate to a Germanic language, one only has to hear English in parts of Canada to see that that is indeed very possible, again in ways that are linguistically recognizable (for that is what substrate or shift-induced interference is).

It is true that a contact-description that, as I put it in Singh (1995), does not provide a full account of the role of Universal Grammar and the grammars of the particular languages involved is more than likely to be not only incomplete but also misleading, but it is also true that there are constraints on what Grammar (in any of its three dimensions) can do. Some of us have been so preoccupied with grammatical constraints on contact that we have forgotten that there are constraints *on* grammar. Be that as it may, what is important to note here is that JTD provides no reason to consider grammar as more important than the (sociolinguistic) histories of speakers who must live with them till they decide or have to change them, taking or giving up (only from what they have, obviously) what they must, clearly within the limits imposed by what we now generally refer to as UG.

As black swans can be very useful for research, it is perhaps a pity that the one presented by JTD turns out not to be black at all. It is also a pity that for someone as interested as JTD in defending the primacy of grammar, she uses a somewhat crude tool-kit to describe matters of phonology, morphology, and syntax, but one needs to sympathize with an author part of whose intended audience needs to be convinced over more than a couple of pages that French could not have contributed an *-s* plural to Belgian Dutch (spoken French generally does not mark its plural nouns in any way, except, of course, in cases involving the substitution of /al/ by /o/).

To conclude, I am perhaps asking for too much on behalf of T&K, but I am convinced that it *will* take much, much more to challenge their eminently reasonable hypothesis than what is offered by JTD. The failure of JTD to make her case, in other words, is not so much a reflection on her attempt as on the formidableness of T&K's challenge. Given that grammar is simply what (sociolinguistic) history operates on, obliging it to supply the materials to build the house or the grave IT wants to build, and issuing, if necessary, a DNR order, it is, in my view, highly unlikely that structuralists (of any stripe) will be able to meet that challenge, but I am happy to leave the matter to history.

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Is it a challenge to the Thomason and Kaufman model?

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Treffers-Daller (henceforth JTD)'s paper is not only interesting but also highly important for general contact linguistics. Admittedly, it did not strike me as particularly noteworthy at first. However, rereading it several times, it eventually made me think. Indeed, the principal merit of the paper is to induce the reader to reconsider some of the more essential questions and problems of theoretical contact linguistics. In spite of being thought-provoking on second sight, it has to be made clear that the paper is in many respects far from being conclusive and convincing. One of its major issues, for instance, is to challenge the widely shared view held by Thomason and Kaufman (henceforth T&K) (1988, p. 35) according to which sociolinguistic factors always take precedence over structural factors in determining the outcome of language contact. JTD disagrees on this point and strongly argues in favor of a total reversal of the hierarchy: to her mind, it is the structural properties of the languages involved in a contact situation which are most important and usually override sociolinguistic factors. At least some of her criticism of T&K's claim seems to be well founded. However, I fail to see how her structure-oriented counter-hypothesis can properly be proved on the empirical basis she presents. As it were, JTD successfully demonstrates that T&K's judgment cannot count as the ultimate word on the determining factors of language contact phenomena, but at the same time her own suggestions remain much too inconclusive to qualify as a viable alternative to the sociolinguistically oriented approach. In what follows, I'll restrict my comments mainly to aspects more or less directly connected to the apparent dichotomy of structural and sociolinguistic models of explanation. Of course, there are many more points to be made which, however, have to be skipped over for obvious reasons of space.

To begin with, several of the more prominent of JTD's concepts do not seem to be defined well enough. As a matter of fact, it is crucial for the understanding of the main arguments of the paper to know what exactly is meant by terms such as, for example, "typological", "outcome of language contact", "sociolinguistic similarity", and so on. Unfortunately, these terms, and others as well, could each have a variety of, at times, rather different readings. In a sentence such as "The similarities in the outcome of language contact find a plausible explanation in the fact that the contact situations are typologically similar", quoted from JTD's concluding remarks, it is by no means evident whether the adverb "typologically" alludes to language type dependent structural properties of the individual languages involved in the contact situation or, rather, to the individual configuration (= "type of constellation") of Romance and Germanic varieties in contact.

For the sake of the argument, let us first suppose that the former is the intended reading. If so, this would amount to saying that typological features such as, for example, on the morphological level, fusional or head-marking and, on the syntactic level, ergative or OV-order, and so on, determine what actually can happen in language contact. On top of that, the second of our hypothetical readings comes close to claiming that genetic affiliation of languages has this determining effect (presumably because genetically related languages are often believed to share a variety of typological features). This, in turn, is tantamount to claiming that the outcome of language contact depends on the languages being members of certain macrophyla, phyla, or subbranches of families. Taking the term typological à la rigueur, I cannot help stating the obvious. All this boils down to a rather trivial observation: the structures of the languages involved in a contact situation determine the outcome of language contact insofar as (a) there must be something there to be borrowed or calqued at all and the donor language usually has it on offer, and (b) there must be a system into which the borrowed or calqued elements have to be integrated. And that is almost the extent of it, for JTD does not provide us with any viable means to determine which structural properties or typological features normally would outrank which other competing properties or features when it comes to language contact. Moreover, one feels impelled to ask whether a competition between such properties or features could ever occur without certain extralinguistic conditions being fulfilled beforehand.

As a matter of fact, JTD need not unconditionally take the blame, for she does not explicitly claim to adhere to any of the abovementioned interpretations. It is not completely out of the way to assume that JTD would happily opt for a less rigid reading of the problematic terms "typological features" and/or "structural properties", especially with regard to what she calls the outcome of language contact. Again, let us suppose that "outcome" is not intended to mean "structural properties acquired via language contact", but rather the level reached on T&K's (1988: 74–76) borrowing scale (there is in fact some evidence for this reading in JTD's abstract and elsewhere in the paper). Nevertheless, there is nothing much gained by this procedure, for one would still have to explain how typological features or structural properties of the languages involved in a contact situation could possibly restrict mutual interference to a certain level of the borrowing scale or enhance its expansion beyond a certain limit. It needs to be clarified

whether or not JTD would subscribe to a hypothesis such as, say, ''the contact phenomena to be observed in situation X have reached level Y because [some of?] the languages involved in the contact situation happen to be of the VO-type''. Without recourse to social factors, such a hypothesis solely based on structural facts must of necessity remain untenable.

One more shortcoming of JTD's paper has to be mentioned here, viz. the inadequate empirical basis. T&K and other authors adduce examples from quite a variety of languages with different genetic, typological, and areal background, in order to demonstrate that neither typology nor genealogy nor area form an unsurmountable obstacle for contact-induced language change. With a view to proving T&K and associates wrong, JTD would have had to discuss many more contact situations with a variety of languages involved. If one aims at showing that typological features or structural properties of the languages involved determine the outcome of language change, one either has to contrast several cases of a different typological make-up or must demonstrate that no matter where on the globe a given language always contributes in the same way to language-contact situations. It is not sufficient to claim in passing that some additional cases involving Germanic and Romance varieties supposedly display a similar outcome of language contact. Rather, one should check to what extent other contact scenarios involving Germanic varieties as, for example, in Swedish–Finnish contacts (Nau, 1995), or Romance varieties as, for example, in Basque–Gallo–Romance contacts (Haase, 1992), yield different results. Quite a few authors adopt a complementary approach: Johanson (1992), Boretzky and Igla (1994), and Stolz and Stolz (1997) study the whole range of contacts experienced by Turkish, Romanes, and Spanish, respectively. With regard to Spanish as a donor language, for instance, it turns out that some 70 indigenous languages of the Americas and Austronesia borrow much the same items from Spanish (mostly function words corresponding to T&K's level 2 phenomena) although the borrowing languages belong to different (macro)phyla, belong to different typological classes, and belong to different areas. In spite of their diversity, the borrowing languages have one thing in common, viz. having been/being exposed to a Spanish superstrate. If one insists on the precedence of structural factors in language contact, the highly similar outcome of language contacts on both shores of the Pacific could only be attributed to the typological properties of Spanish. Wouldn't that entitle us to reformulate JTD's claim in much stronger terms, saying that it is the structure of the donor language which determines the outcome of language contact? Or isn't it rather the case that the macro-sociolinguistic situation in the former Spanish colonial empire and its modern successor countries was and, in the Americas at least, still is much the same all over the place and, therefore, created similar conditions for borrowing from Spanish no matter which indigenous language was involved?

Speaking of (macro-)sociolinguistic similarity, more problems with JTD's way of seeing things arise. Obviously, much of her argumentation rests upon proving that the sociolinguistic situations in Brussels and Strasbourg are at variance. In a way, JTD seems to set different standards for establishing the similarity/dissimilarity of macro-sociolinguistic patterns, on the one hand, and of languages involved in contact situations, on the other hand. Obviously taking the similarity of Germanic varieties (or Gallo-Romance ones, for that matter) for granted, JTD meticulously scrutinizes a variety of sociolinguistic parameters on all of which Brussels and Strasbourg, more or less, appear to behave differently, though perhaps not to every reader. It is almost a truism that the more parameters one chooses to include in the description of two subjects as, for example, two sociolinguistic situations, the more likely it becomes that the two will turn out to be essentially different in nature. The same no doubt would also apply to languages and their structures, so that on equally close inspection Brussels Flemish and Strasbourg Alsatian could have become as dissimilar on the structural level as Brussels and Strasbourg are said to be on the sociolinguistic level. It hardly needs to be mentioned that the list of parameters JTD draws upon T&K's classic could easily be made look different. In my opinion, one criterion that somehow went missing is the reading (maybe even writing) competence that speakers of Germanic substandard varieties in Brussels and Strasbourg have in the standard varieties of Dutch and German, respectively. At one point, JTD mentions German as ''the written code (Schriftsprache) in Alsace'' between 1871 and 1919, but does not include standard variety reading and writing competence in her catalogue of parameters, although I suspect that (a) such skills can hinder massive borrowing, and (b) such skills are likely to play a role both in Brussels and in Strasbourg. In order to keep things short, I will only point out one more problem. In spite of a short flashback into history, the macro-sociolinguistic picture JTD paints of the two cities is essentially a synchronic one. However, T&K (1988: 35) explicitly speak of the sociolinguistic ''history'' of the speakers. Indeed, when analyzing a sociolinguistic situation as setting for language contact, one should adopt a perspective covering at least the lifetime of two or three generations. Taking this extended perspective, one easily notices that standard German has been present in the everyday life of Strasbourg and elsewhere in the former Reichsland Elsaß-Lothringen (Stroh, 1993), that is until relatively recently Alsatian was not a proper roofless dialect! The repeated annexations and reincorporations of the territory were accompanied by anti-French or anti-German language campaigns which are somewhat reminiscent of the Flemish–Wallonian conflicts – though not completely. In the Alsatian region, standard German was the official language and only means of instruction from 1871 to 1919 and in 1940–1945. It is highly likely that this historical fact bears on the linguistic behavior of today's elder generations in Strasbourg.

Both contact situations are of long standing. Some of the potential examples of borrowing from French in either Brussels Flemish or Strasbourg Alsatian which JTD discusses may date back far in time, and it is therefore methodologically unsound to count those cases as recent

borrowings. Take for instance the verb exküsieren (not mentioned by JTD's source?) attested in Alsatian. You won't find this form in dictionaries of modern standard German, but it was quite common in Germany in the seventeenth and eighteenth centuries. As is the case with many German dialects on the left bank of the Rhine, quite a few lexical gallicisms have been preserved whereas they have been lost from the standard variety. In a number of cases, some of the putative local borrowings from French have identical equivalents in the standard varieties as, e.g., Brussels Flemish raritajt = ABN rariteit, Brussels Flemish = ABN sympatiek, Brussels Flemish = ABN attentie. The different rules concerning ge- prefixation on participles of French-derived verbs in Brussels Flemish and Strasbourg Alsatian correspond exactly to the rules that are operative in the standard varieties of Dutch and German: gearrangeerd vs. arrangiert. This seems to suggest that the influence exerted by the standard varieties on the Germanic substandard varieties should not sweepingly be belittled.

In addition, there are some minor points worth mentioning: (a) Since diminutives always take plural-s in Dutch irrespective of the plural affix usually required by the non-diminutive base form (huis – huiz-en vs. huis-je – huis-je-s [house]), the use of -s on manchkes is not really valid proof of the Dutch origin of -s on *manches (However, JTD correctly mentions more compelling reasons for deriving "pronounced" -s on French loans from Dutch); (b) It comes as no suprise that a complex conjunction like à moins dat triggers normal subordinate clause word order in Brussels Flemish, for it is only partially French containing subordinating Dutch dat; (c) The use of Dutch van instead of over in combination with the verb spreken could also be an extension of a pattern established in ABN in expressions such as, for example, goed van iemand spreken (to speak well of someone) and daar is geen sprake van (that is not an option).

In conclusion, it turns out that the riddle of what comes first – structure or social factors – remains to be solved. It cannot be denied that both interact but it still needs to be established which of the two is the primary force. Maybe the function of prime mover varies according to some higher-level parameter. Interestingly, T&K's non-English case studies (1988, pp. 215–262) involve preliterate or only partially literate societies, whereas JTD is concerned with highly literate groups only. One should investigate the extent to which literacy bears upon the outcome of language contact. For the time being, the structure-oriented alternative put forward by JTD has hardly more predictive power than competing approaches. I'd like to encourage JTD to carry out her planned project on language contacts along the Germanic–Romance linguistic border. However, I also strongly recommend that some contrastive studies should be included which involve languages of a different genetic, typological, and areal background.

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On predicting calques and other contact effects

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Jeanine Treffers-Daller (JTD)'s comparative analysis of two French–Germanic contact situations offers valuable insights into the kinds of factors that can determine the linguistic results of contact. Case studies like this are exactly what is needed to increase our understanding of contact effects. In these comments I will urge caution in drawing general conclusions about the relative weight of social and linguistic factors in predicting the effects of contact; I believe that we need more case studies, worked out in comparable detail, before our chances of drawing valid general conclusions about the interplay between linguistic and social factors will be very good. But my comments are not meant in any way to detract from her achievement in this and related research projects. By providing careful arguments and evidence to support her proposals about the Brussels and Strasbourg situations, she has contributed much to our overall knowledge of these issues.

My starting point is JTD's final sentence: "Thus, the facts described in this and previous papers ... lend support to the claim that structural factors rather than sociolinguistic factors are the primary determinants of the linguistic outcome of language contact." This sentence follows her convincing demonstration that, because they differ strikingly in the two contact situations, social factors – especially what might be called the usual suspects in macrosociolinguistics, as laid out in her Table 2 – do not help to explain the parallel developments in the languages of Brussels and Strasbourg. But the fact that linguistic factors are the best predictors of these results does not justify the conclusion that linguistic factors are the best predictors of contact effects in all situations. My own opposite belief, that social factors are the primary determinants, does not rest on the premise that linguistic factors will inevitably be secondary in every contact situation. Instead, it derives from the observation (based primarily on a survey of the language-contact literature) that social factors can and do override linguistic factors when the two would predict different results – but not vice versa.

In a sense, of course, the claim that social factors are primary is trivially true: language contact itself is a social fact, not a fact of language structure. No contact, no contact effects. More significantly, the dichotomy that JTD appeals to in interpreting her results – interference through borrowing by fluent bilinguals, as opposed to interference that takes place as a result of imperfect learning (a.k.a. shift-induced interference) – is a social dichotomy with linguistic results, not a linguistic dichotomy.

Moreover, the borrowing scale that Kaufman and I presented (1988) is based on the admittedly vague concept of intensity of contact, which (whatever it is) is also a social factor. To some extent, of course, its justification is obvious: in a borrowing situation, greater intensity of contact means more bilingualism among borrowing-language speakers, and that in turn helps to explain why structure is borrowed only under more intense contact conditions: you can't borrow things you don't know, so a reasonable level of bilingualism among borrowing-language speakers is needed before source-language structural features will be widely enough known to be borrowed. In her words, the linguistic factors that determine particular types of change can't begin to operate until the social requisites for change are met. All these social factors are the same in both of JTD's contact situations. The main conclusion I would draw from her results, therefore, is that the social factors she lists and compares in Table 2 are not, or at least not in these instances, determinants of the linguistic outcomes.

If this were all, however, a believer in the primacy of linguistic determinants could simply respond that linguistic factors will always be primary *after* one gets past the first trivial condition (there has to be contact for contact effects to exist) and the more significant but still very general social determinants that are identical in JTD's two contexts (borrowing vs. shift-induced interference, and enough bilingualism to permit structural borrowing). The respondent could even stipulate that there might be a few other crucial social conditions, but that at more detailed levels of analysis, linguistic factors will always win out over social factors. But I don't believe that this is true, either. It holds for the two situations that JTD compares here, but it doesn't hold in all cases.

As an example, consider the problem of calques and loan shifts. For the data I'll discuss, these two closely related phenomena can be conflated. They do of course differ – calques involve morpheme-by-morpheme translation from a complex source-language word or phrase, while loan shifts are semantic interference alone, in which a foreign meaning replaces the native meaning of a native word. But they also share a crucial feature: both are importation of meaning without lexical form. Only calques may involve structural interference as well, and they do so when the complex source-language structure is structurally different from receiving-language structure and the calque preserves the source-language pattern. In discussing this type of interference for Brussels and Strasbourg, JTD shows that it is more common in the French varieties than in the Germanic varieties, and concludes that it is "an additional typical characteristic of shift-induced interference".

This conclusion strikes me as overhasty, because there are contact situations in which bilinguals borrow lexicon, or at least lexical semantics, by calques and loan shifts

more often than by direct importation of words. The case I know best is Montana Salish, a member of the Salishan language family; but the borrowing behavior of some other Native American tribes of the Northwest Territories, for instance Nez Perce, is similar in some respects and perhaps also in this one.

Montana Salish is a seriously endangered language, with only about 60 fluent speakers at present, almost all of them over 60 and most over 70 years old. All these speakers are fluent bilinguals; aside from a few characteristic "Native" features, particularly intonational patterns, their speech is indistinguishable from that of most other American speakers of English. English has been an important part of the tribe's linguistic environment for well over a hundred years, and full bilingualism among native speakers of Salish has been well established in the community for several decades at least. As with other Native American tribes, the Montana Salish people have been under intense cultural pressure from the dominant Anglo society for a long time. They have acquired all the material trappings of Anglo America. But, strikingly, they have not borrowed the words along with the borrowed objects and less tangible cultural attributes. With few exceptions, mostly place names, English loanwords are largely lacking in Montana Salish. Instead, speakers construct names for new things out of native morphemes and in ordinary native constructions. A typical example is the Montana Salish word for automobile, which is *p'ip'úyšn* – literally "wrinkled feet", a name derived from the appearance of tire tracks. (The word for one car tire is *p'úyšn*, literally "wrinkled foot".)

Sometimes, however, Montana Salish speakers do borrow the meanings of English words and phrases, through calquing or loan shifting – most often the former. One example is the Montana Salish word *súť-s* "long face" (literally "stretch-face"), as in k^w *súť's* "you've got a long face, you look sad, mean". According to current elders, this word is a literal morpheme-by-morpheme translation from English – a calque. Another example provided by the elders is viewed as a joke, not used in ordinary Salish speech but only as an interlingual pun (and maybe used only once): k^w *póc-qn*! "you're a sorehead!" (literally "you sore-head"), which was said early in this century by an 8-year-old boy (who had learned English in school) to his mother when she was being grouchy. The process is certainly borrowing, not shift-induced interference.

An especially elaborate example of calquing in Montana Salish, of entire sentences, was offered several years ago by a 98-year-old speaker. In eliciting data to check certain morphological constructions, I requested translations of sentences like "Alye stole the huckleberries from Mali". To my surprise, my consultant gave sentences which, though fully grammatical in Salish, lacked all the elaborate transitive apparatus of the usual forms that are given as translations of such English sentences and that occur naturally in Salish texts. He provided intransitive forms instead; in Salish, intransitive verbs can occur in construction with objects, though such constructions are stylistically marked. Stripped of all the transitive morphology, the forms were relatively analytic, like English; and he also used English word order, which is possible though stylistically marked in naturally occurring Salish. The resulting sentences closely resembled a word-for-word translation of the English sentences – that is, syntactic calques, exploiting the rich possibilities of Salish structure to match the English structure as closely as possible. When I finally asked if he would normally say those things in that particular way, he said no, of course not, but since I had asked for translations of the English sentences, he thought that I wanted something like English.

My point here is not that this speaker was spearheading a massive rearrangement of Montana Salish structure into English-like sentential calques. Not only have I never heard such things from other speakers, but even this speaker never offered such sentences before or after that one occasion. My point, rather, is that this speaker had the knowledge required to produce English-like calques in his bilingual repertoire, in case such knowledge was wanted; it is essentially the same capacity that other bilingual speakers have used to produce temporary and permanent lexical calques in Montana Salish. This knowledge may be used in this particular community more often than in others. If it is, the social reasons are opaque; but they must in some sense be attitudinal. Similarly, the lack of numerous English borrowings must also be due to the speakers' attitudes.

I believe, on the basis of examples like the Montana Salish calques and other examples elsewhere of linguistic creation (especially cases of deliberate change), that speakers' attitudes – their conscious or semi-conscious or even unconscious decisions about how to exploit their bilingual or multilingual repertoires – are fundamentally unpredictable. Certainly they are unpredictable on linguistic grounds; language structure is not a deciding factor in the choice to acquire new words through borrowing or calquing or original creation from native morphemes. (It's true that claims have been made to the effect that some things are unborrowable in some languages, but all these claims can easily be counterexemplified, and indeed have been.) Speakers' attitudes are mysterious, and I don't pretend to understand them. But it is at least clear that attitudes are social factors, not linguistic ones; and it is equally clear that they can and do outweigh any strictly linguistic predictions about what will happen in a given type of contact situation.

As I noted at the beginning of these comments, this reservation about Treffers-Daller's general conclusion has no effect on my assessment of the importance of the results of her investigation. Her analysis is excellent, carefully reasoned and insightful, and her study is a major contribution to language-contact research.

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Throwing the baby out with the bathwater?

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Treffers-Daller (henceforth JTD) offers us a very informative account of the patterns of contact-induced change in the French and Germanic varieties used in two communities, Brussels and Strasbourg. We obtain a fairly clear picture of the nature and extent of borrowing from French into varieties of Dutch and Alsatian, as well as the types of L1 interference which the latter exert on French, which historically has been acquired as an L2 by (all?) members of the two communities. The study is one of a number of rare attempts to provide a comparative perspective on contact situations and their outcomes. Such studies are vitally needed to provide a base for establishing a unified framework and model for contact-induced change in general.

JTD finds ample support for Thomason & Kaufman (henceforth T&K)'s hypothesis that cases of (slight to moderate) borrowing usually result in more extensive influence on the recipient language's vocabulary. Structural borrowing occurs only later, as a result of greater "intensity of contact". On the other hand, as T&K predict, in cases of language shift (or untutored second-language acquisition) "interference" from learners' L1 on the target of shift shows up more in phonology and syntax than in vocabulary. The results reported by JTD support these predictions fully. But the crux of JTD's finding is that, despite differences in the contemporary sociolinguistic situations in Brussels and Strasbourg, "the overall contact patterns are very similar, both from a quantitative and from a qualitative point of view" (page 1). In other words, in both situations, we find similar degrees and types of borrowing as well as "interference through shift" (otherwise referred to as "L1 transfer" or "substratum influence"). This leads JTD to challenge T&K's well-known assertion that "it is the sociolinguistic history of the speakers, and not the structure of their language, that is the primary determinant of the linguistic outcome". Rather, she argues that her findings support the opposite conclusion that "it is the structure of the languages involved rather than the sociolinguistic history of the speakers, which determines the outcome of language contact in the first place" (page 1).

JTD raises here a very important issue that is just beginning to engage the serious attention of language-contact researchers. The issue was perhaps best outlined by Weinreich, who suggested: "In linguistic interference, the problem of major interest is the interplay of structural and non-structural factors that promote or impede such interference" (1953, p. 5). The operative word here, of course, is "interplay". Neither Weinreich nor T&K were prepared to separate the effects of purely linguistic as opposed to sociocultural factors, and both agreed that the latter were in fact an indispensable element of any attempt to describe the extent, direction and nature of influence of one language on another. T&K took this a step further, by assigning primary importance to sociocultural factors.

While JTD is right to question this claim, one wonders whether her zeal has led her to create too strict a dichotomy between structural and nonstructural factors as determinants of contact outcomes. Such a clear separation was clearly not intended by Weinreich, and T&K's wording of their hypothesis – that sociolinguistic history is the *primary* determinant – certainly does not rule out a role for linguistic factors. JTD, on the other hand, by using the phrase "structure ... *rather than* sociolinguistic history" (my italics), seems to minimize, if not rule out altogether, the relevance of the latter. It seems only reasonable to expect that, on the one hand, contact situations which resemble each other greatly in their sociolinguistic make-up but differ significantly in terms of the typological relationships between their linguistic inputs will yield somewhat different outcomes, all else being equal. And, on the other hand, the same would be true of situations which are sociolinguistically different but involve closely related linguistic varieties. One of the strong challenges to language-contact theory is to identify how different constellations of such structural and non-structural factors contribute to different outcomes. JTD must at least be commended for tackling this complex issue.

But her approach raises several unanswered questions concerning the precise bases and criteria we should use in such an undertaking. For instance, what criteria do we apply to establish the degree to which contact situations are in fact similar in sociolinguistic history and contemporary sociocultural organization? JTD claims that there are "major" social differences between Strasbourg and Brussels, despite some apparent general similarities. In particular, she claims that there are differences in terms of "level of bilingualism, length of contact, relative sizes of speaker populations and socio-economic and/or political dominance of source-language speakers over borrowing language speakers" (page 3). These reflect, of course, the rough set of criteria that relate to T&K's concept of "intensity of contact" as a major factor in determining contact outcomes. But, apart from listing these differences in the two sociolinguistic situations, JTD does not offer any explanation as to why they *should* have led to greater differences in the patterns of contact-induced change in the two communities than they actually have. How diagnostic are such differences in the first place, and how sharp must they be to have markedly different effects? On the face of it, the two communities in fact appear much more similar than the differences suggest. Both involve language maintenance of a native vernacular with bilingualism in a politically imposed standard variety. They are similar too in terms of their sociolinguistic histories, the assymetrical

prestige relationships between the French and Germanic varieties, and so on. Another difficulty is the fact that, as JTD acknowledges, her approach is "macro-sociolinguistic" rather than "micro-sociolinguistic". Hence a great deal is missing from her picture of the social situations. Can we so easily overlook the micro-level factors, including social network structures, individual and group relationships, language histories, and especially individual attitudes and choices of identity that might be relevant to language use in these communities? JTD herself acknowledges that such micro-level factors do "have a bearing upon the frequency with which individuals display the language-contact features". The question, then, is to what extent do similarities and differences at the micro-sociolinguistic level reflect similarities and differences in language behavior in the two communities? We find little discussion of the former, but there is evidence of differences. For instance, JTD notes that code-switching is less widespread in Brussels than in Strasbourg, and less frequent in the former city among speakers who know mainly Standard Dutch and Belgian Dutch, as well as among younger Brusselers. If we regard CS as an outcome of contact, we seem to have here evidence of social differences (e.g., ethnolinguistic conflict in Brussels, divided loyalty to French and Standard Dutch, age differences, and the absence of bilingual schools) leading to linguistic differences.

So, the similarities that JTD focuses on have to do mainly with the nature and extent of change in the varieties under contact. The relatively slight mutual influence of the languages in contact in these two bilingual situations is admittedly surprising, and testifies to relatively strong language loyalty and resistance to external influences, despite what appears to be intense contact. JTD is right to ask why this is so. But instead of focusing only on the apparent social differences, we need to explain also what similarities in sociocultural and sociopyschological organization of linguistic means might be responsible for the preservation of the relative autonomy of the varieties in contact. A comparision such as this must resort to well-established frameworks for the investigation of the speech economy of the communities in question, especially the frameworks of the Ethnography of Communication and Social-Psychological models such as Speech Accommodation theory. Hymes's (1967) situational typology might well be wedded to Giles's (1988) taxonomy of "communicators' ideologies and construals of the social structure" to yield a comprehensive picture of "communicator characteristics in intercultural settings" (Giles, 1988, p. 363).

It is a pity that JTD offers only a cursory account here of language ideology and language attitudes in the two communities. Scholars have long emphasized the impact that psychological factors such as the symbolic value of language varieties for different speakers can have on the outcomes of contact. And in fact, both Weinreich and T&K place language attitudes on a par with other social factors as vital to the nature of the outcomes of contact. So JTD's focus on "intensity of contact" to the exclusion of ideologies and attitudes is somewhat one-sided and unfair to T&K. Finally, to be completely fair to T&K, the list of factors they proposed as relevant to the role of "intensity of contact" was meant to be a preliminary one, and they emphasized that the task of identifying the relevant sociocultural factors and comparing the effects of different sociolinguistic profiles was one for future research by the relevant sociolinguistic disciplines. JTD's attempt here is valuable in reminding us of the challenge facing us in this respect. We would profit much from comparing contact situations such as those in Brussels and Strasbourg not just with each other, but with other bilingual situations involving varying degrees of L1 maintenance and shift to a superposed standard. This would cover situations as different as those that have given rise to heavy borrowing, convergence, and the creation of bilingual mixed languages.

Contemporary situations such as those in Singapore and Taiwan, where distinctive contact vernaculars are emerging as markers of new local identities, and in which the effects of language contact can be directly observed, provide very interesting points of comparison and contrast with places such as Brussels and Strasbourg, where language boundaries tend to be maintained. Ultimately, our comparisons must include situations involving shift accompanied by language death, as well as the kinds of language creation involved in pidgin and creole formation. It is surely beyond dispute that social and other non-structural factors play a major role in shaping the outcomes of contact in such diverse situations. Even within the class of so-called "creoles", it has long been recognized and demonstrated that differences in the community settings, demographics, codes of social interaction, and ideologies make for dramatic differences in the outcome, often despite similarities in the linguistic inputs and their typological relationships. The task before us is to meet T&K's challenge to identify the range of relevant factors. As Giles notes, the major problem for our attempts to compare contact situations is the fact that different investigators focus on "isolated sets of independent and dependent variables, some with attention to communicator beliefs and expectations, and others to particular sets of sociolinguistic behaviours in quite disparate settings" (1988, p. 363). For these reasons, as well as because of gaps in our information about the socail context, different studies are often not comparable. What is needed, then, is an "agreed-on blueprint of contextual features which form the backdrop to sociolinguistic behaviour so that intercultural comparisons are really viable" (1988, p. 364). JTD's effort is at least a step in this direction.

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AUTHOR'S RESPONSE

The nuts and bolts of language contact

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I would like to begin by expressing gratitude to all scholars who have contributed to the discussion by writing a comment on my paper. It certainly is a rare privilege to receive such interesting comments from colleagues from all over the world. It is also fair to say that I have profited tremendously from all the helpful comments of a number of colleagues who read my paper prior to publication. The peer comments have helped me to rethink a number of important issues I have raised in my paper, and I am grateful to the editors of the journal for giving me the opportunity to share my reflections on the comments with all readers interested in these issues. The comments focus on a number of central points that I would like to discuss in the following order. In the first place, there is the issue of terminology. Different commentators express the need for a clarification of the meaning of Thomason and Kaufman (henceforth T&K)'s famous notion "sociolinguistic history of the speakers", whereas others feel that the notion "linguistic outcome of language contact" remains unclear. In the second place, several commentators wonder when two bilingual communities can be considered to be similar from a sociolinguistic point of view and whether I am clear enough about the issue of structural similarity or typological distance between languages (Singh, Stolz, and Winford). Third, I would like to go into the issue Beeching raises regarding possible internal explanations for the phenomena I discuss. Fourth, some commentators mention the possibility of explaining the facts with the help of other models, such as Le Page's concepts of focusing and diffusion (Sebba) or Giles's notion of ethnolinguistic vitality (Poplack and Meechan). Fifth, there is the issue of generalisability of the analysis (Singh, Stolz) to other Romance–Germanic communities along the linguistic frontier or elsewhere. The sixth point relates to Sebba's observation that I analyse data from societies with a high degree of literacy, whereas the data T&K focus are often from societies with a far lower literacy percentage. I will finish by making a few remarks regarding directions for further study.

Some terminological issues

I was surprised to read Poplack and Meechan's comment that neither T&K nor I have made explicit or operationalised which factors are pertinent to identifying the sociolinguistic history of a speech community. I believe T&K very clearly, although briefly, point to a number of concrete factors identifying the sociolinguistic history of the speakers, such as demographic factors, institutional support for different varieties, intergroup attitudes, the role of standard languages, and so on. The list in T&K is not complete and is only intended to be indicative of the sort of factors we must look for. Sebba's observation regarding literacy levels is an important constructive contribution to this list. Most of the factors T&K mention pertain to the macro-sociolinguistic level of analysis, and many sociolinguists are involved in the analysis of micro-sociolinguistic factors. As I set out to test T&K's theory I have concentrated on their operationalisation of the notion of sociolinguistic history in the first place. I believe it is justifiable to take a macro perspective rather than a micro perspective because I want to compare bilingual communities as a whole and I do not single out individuals or subgroups from particular social networks, age groups, social or regional backgrounds. I have clearly said that micro sociolinguistic factors such as the ones just mentioned influence the outcome of language contact in the sense that speakers from a particular area in Brussels may display more contact features than speakers from another area. More details about the sociolinguistic conditioning of contact phenomena can be found in my book (Treffers-Daller, 1994). The aim of the paper is thus not to deny such influences. I fully agree with Thomason's remark that social factors are primary in the sense that language contact is necessarily a social fact, and that the dichotomy borrowing-interference through shift is a social dichotomy in the first place. Clearly in language contact it is speakers who meet and not abstract entities. Once these first prerequisites are met, and they are obviously met in Brussels and Strasbourg, we can start looking for relevant differences between contact situations. In the case of Brussels and Strasbourg, there are important differences, as I have pointed out in my paper. Large-scale surveys of Strasbourg and Brussels comparable to the work Poplack and associates have carried out in Canada would be necessary to further identify the interaction between macro and micro factors in language contact. Unfortunately such surveys do not exist for Brussels and Strasbourg. On the basis of the evidence currently available I can only say that the differences are of a quantitative nature rather than of a qualitative nature. I found more borrowing in the inner city of Brussels than in Anderlecht, but the integration patterns of these borrowings were the same. On the other hand, I found important qualitative differences between the integration patterns of French past participles in Alsatian and Brussels Dutch (Treffers-Daller, 1997 and in press). These differences can be explained on the basis of structural

differences between Alsatian and Brussels Dutch. Thus, there is a qualitative difference between the outcome of language contact with respect to at least one point: the past participles. And sociolinguistic factors – whether macro or micro – have little to contribute to an explanation of these facts.

I fully agree with Stolz, who points out that there are probably other differences between the grammars of Brussels Dutch and Alsatian. With respect to the phonological systems of both languages I expect there to be interesting differences which will probably lead to exciting differences in the phonological integration patterns of French words into either language. There is probably a world of interesting facts to discover here. That is precisely the point I have tried to make throughout: if there are structural differences between Alsatian and Brussels Dutch I expect these two languages to treat French borrowings/code-switches differently. In other words, the linguistic outcome of language contact is different in both cities, due to the structural differences between the receiving languages.

The linguistic outcome of language contact

Having arrived at this point it is necessary for me to say a few words about the interpretation I give to the notion "linguistic outcome of language contact". I should like to say that this is my personal interpretation of this notion. I can only hope that my interpretation is close to the reason why T&K created this famous expression. In their book T&K concentrate on systematising the linguistic facts rather than the sociolinguistic facts; there are obvious reasons for their choice. Many sociolinguistic facts are simply not available because the data discussed are historical data. In the case of Brussels and Strasbourg we are a lot more privileged because we can look at both sides of the coin: on the one hand I have tried to compare systematically the sociolinguistic facts, and I have tried to make the point that there are important differences between Brussels and Strasbourg from a sociolinguistic point of view, despite the apparent similarities. On the other hand I have summarised the main linguistic facts and here the similarities are striking. We can see the sociolinguistic facts (macro and micro) as the independent variables and the linguistic facts as the dependent variables, as is often done in sociolinguistic studies. The macro-sociolinguistic factors do not act directly upon an individual's language behaviour, but it is in complex interactions with micro-sociolinguistic factors that their influence can be seen. If we take this point of view we can say that the macro- and micro-sociolinguistic factors influence the outcome of language contact. But what exactly do these factors influence? We can see their influence in the fact that speakers living in Anderlecht use fewer borrowings than speakers in the inner city of Brussels. We can also see their influence in the fact that code-switching is not a community-wide phenomenon anymore in Brussels: there are too many tensions between the language groups and this makes it hard for speakers to identify with both languages, as I have argued earlier (Treffers-Daller, 1992). It is also perfectly clear that power relations between the Romance and the Germanic speaking populations determine who borrows from whom (see also Singh's peer comment). Even though I fully agree with all of this, I also maintain that sociolinguistic factors have no influence on structural matters such as the one discussed above regarding the structural integration of French past participles in Brussels Dutch and Brussels French. I take it that no one would like to argue that sociolinguistic factors have any bearing upon these facts. The same is true for the plural formation on French borrowings in Brussels Dutch. There are compelling arguments for considering the -s on *camions* (lorries) as a Dutch -s, rather than a French -s. The relevant point to be made here is that the arguments put forward for this discussion are of a strictly structural nature, and I don't think anyone would like to argue that sociolinguistic factors play any role in this. Singh formulates the distribution of labour between sociolinguistic and structural factors in language contact very clearly in his peer comment by saying that structural considerations supply the materials and the paths to be followed in language change, whereas the sociolinguistic history of the speakers decides the destinations of language change. Given this distribution of labour, why do I wish to consider linguistic factors as having priority over sociolinguistic factors? Because I believe that structural factors set the parameters or the options available to the speakers. The speakers can choose to use all the options or to use only some of them. Which options they choose will depend on their position in the sociolinguistic context.

Stolz is right in asking me to identify the structural factors which set the parameters. Is it language-type dependent structural properties of the languages involved in the contact situations? Or is it the individual configuration of Romance and Germanic varieties in contact? My answer is going to be disappointing, because I don't know. My first reaction to this is that I would like to investigate further what is happening along the linguistic frontier before I go into wild speculation about typological characteristics of contact languages in general. I prefer to keep my feet firmly on the (Germanic and Romance) ground to sort out the facts. But I am sure that insights from comparative typological studies are going to be extremely valuable if we want to advance in this field.

Internal versus external explanations

Beeching comes up with interesting new facts about spoken French, which indicate that the fronting of direct objects and especially of *ça* is fairly frequent in other varieties of spoken French, not influenced by either Dutch or German. As T&K have pointed in their book, there almost always are both internal and external explanations for certain facts. Traditionally researchers have tried to downplay external factors and have tried to argue that internal factors alone are responsible for the facts. T&K show that this view cannot be maintained and that externally motivated language change is found everywhere where languages come in contact. Some researchers have even argued that all language change is externally motivated, because

changes are often the result of contact between different speakers, belonging to different social classes or age groups or between speakers living in different areas. T&K take an eminently sensible point of view by assuming that different causes can collaborate to produce a particular phenomenon (multiple causation) and I assume that that is also the case here. Does the fact that fronting of direct objects can (also) be explained on the basis of internal facts invalidate the general argumentation of the article? No, I don't think so. There is a clear contrast between the syntactic influence from French on the Germanic varieties – which is linked to lexical borrowing – and the syntactic influence of the Germanic varieties on French – which is not limited to or linked to lexical borrowing at all. Beeching claims that it is implausible that French varieties should adopt structural items without first adopting lexical items. From the literature on (untutored) Second Language Acquisition as well as from the literature on language shift it is clear that L2 learners generally display phonological and syntactic interference from L1 in the target language. Vocabulary items from L1 are far less likely to occur in L2 speech (see also Winford's peer comment). Moreover, phonological and syntactic features appear in target language structures not containing any vocabulary items from the source language (cf. T&K, 1988, pp. 114–115). T&K's model thus makes an excellent prediction for Brussels and Strasbourg and the data from these cities provide clear support for their basic distinction between borrowing and interference.

Similarity and difference

A number of commentators have asked for clarification of the notions "similar" and "comparable". I agree that these are slippery notions and a standard of comparison would be extremely useful here. Some commentators consider Brussels and Strasbourg to be fairly similar despite all the differences I have listed. It is true that Strasbourg and Brussels have more in common than, say, Brussels and Lubumbashi. In an ideal world we would like to see a comparative analysis of French–Dutch contact in Brussels and French–Dutch contact in southern America or French–German contact in Strasbourg and French–German contact in for example Congo. Needless to say, we are running into problems when we are looking for such data. If such comparisons are possible in other language pairs, as Stolz suggests, that seems to me to be a very fruitful line of research, if both the sociolinguistic and the structural facts are analysed in some detail. I personally believe that the contact situations along the linguistic frontier are sufficiently different – from a sociolinguistic point of view – to come to interesting conclusions. I do not agree with Sebba's point of view that French, Dutch and Alsatian are all typologically similar in significant ways. The pronominal system, the gender classification, the basic word order and the tense and aspect system of French differ considerably from Dutch and Alsatian, to name just a few very conspicuous points.

The whole discussion about similarity and difference becomes even more complex when one realises that a lot depends on speakers' perceptions of similarities and differences. As Auer puts it, "There may be cases in which the two systems juxtaposed by the members of a community (and interpreted as such) are 'objectively speaking' very similar, but from the members' point of view completely independent (as in dialect standard switching), just as there may be systems that are 'objectively speaking' very distinct but nevertheless seen as non-distinct by the users" (1998, p. 13). The search for objective standards for evaluating similarity and difference is therefore bound to be a very difficult one.

Other theories: Le Page and Giles

Sebba suggests that Le Page's theory may shed an interesting new light on the contact patterns in Brussels and Strasbourg. Although I am happy to believe that highly focused norms impede borrowing, it is not clear to me how this explains the contrasting borrowing and interference patterns in Brussels and Strasbourg. There is considerable evidence that Dutch and French norms are considerably focused in Brussels. Purist attitudes associated with the standard languages are omnipresent. Despite the existence of such norms Standard Dutch borrowed a lot of words from French. It is also fairly unlikely that such focused norms exist for Brussels Dutch, and I would doubt their existence for Brussels French. As Sebba indicates, the existence of focused norms can perhaps explain the absence of grammatical borrowing, but it doesn't explain the pervasive presence of lexical borrowing in the Germanic varieties and the widespread phenomenon of loan shifts in the French varieties.

Poplack and Meechan suggest that Giles's notion of ethnolinguistic vitality would be of more help in explaining the differences between the language communities in Brussels and Strasbourg. As I have detailed all the relevant factors Giles considers to contribute to a group's ethnolinguistic vitality, I feel that I have precisely fulfilled their request, albeit without using the words "ethnolinguistic vitality" explicitly. Poplack and Meechan are right in pointing out that some factors contribute to enhancing the vitality of Dutch in Brussels, others may work in the opposite direction. Whether this leads to more or to less similarity between Brussels and Strasbourg remains entirely open. It depends on the weighting of the individual factors, and I don't think we can find a metric for saying how much weight we are to give to the factor "institutional support" in comparison with the factor "conflict". To my knowledge, Mackey (1976) is the only researcher who tried to propose a model for measuring and quantifying differences between language groups on demographic, cultural and economic variables. I don't think anyone has tried to follow his example. His efforts, however admirable in their intention, are doomed to fail because individual speakers and speaker groups from the same bilingual community will probably differ in their evaluation of the importance of these factors too.

Language contact and literacy

Sebba raises a very interesting point, namely the influence of schooling and literacy on the occurrence of language-contact phenomena. Nowadays in western Europe, literacy levels are very high, both in comparison to the situation in these countries in the past and in comparison to the situation in other parts of the world. To give just one example of literacy levels in a west European country, according to Gläss (1988) between 0.5% and 3.5% of the population in Germany is illiterate. Most of the contact situations T&K report on have far lower levels of literacy. One may wonder whether literacy has a constraining effect upon the amount and the type of language contact found in speech communities with a high level of literacy, such as the contact situations in Brussels and Strasbourg. As borrowing and interference remain relatively restricted, and literacy is widespread, this seems to indicate that literacy may have been a factor in constraining language contact. Daller's (1997) study of language mixing in an illiterate Dutch–German bilingual gives some support to this idea. Daller found very complex patterns of language mixing in this bilingual and hypothesised that illiteracy can promote mixing of languages, because the permanent visual support for the differences between the two languages is lacking. As German and Dutch are very closely related languages, the difference between them is sometimes visible in writing only (e.g. German Saal, Dutch zaal – English hall). It is also interesting to note here that one of the most fluent code-switchers in my Dutch–French data was illiterate. Clearly more data about language mixing among illiterates must become available before we can draw any further conclusions. But it seems to be a very interesting line of research.

Where do we go from here?

I believe new relevant facts about the interaction between social and structural factors in language contact can be found through a comparative analysis of the contact patterns along the linguistic frontier. The contact situations are sufficiently similar to make a comparison possible, and sufficiently different – from a sociolinguistic point of view – to make a such a comparison interesting. I predict that the most interesting qualitative differences in contact patterns will be found when there are structural differences between varieties, such as the difference in integration patterns for French past participles in Alsatian and in Brussels Dutch. The integration patterns for past participles are different because the rules for past participle formation in Alsatian and Brussels Dutch differ in subtle ways. It is likely that more such subtle differences will be found in further studies of contact situations along the linguistic frontier, and I am very confident that these analyses will shed new light on the interaction between structural and sociolinguistic factors in language contact.

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